1854MNRAS..15....1H

ROYAL ASTRONOMICAL SOCIETY.

Vol. XV. *November* 10, 1854. No. 1.

G. B. Airy, Esq. President, in the Chair.

Wadham Lock Sutton, Esq., Berkhampstead, Herts, was balloted for and duly elected a Fellow of the Society.

Vol. xxiii. of the *Memoirs* has recently been published. It contains three plates of engravings, besides a woodcut illustration accompanying one of the papers. The price of the volume is 4*s*. to Fellows of the Society and 8*s*. to the public. Vol. xiv. of the *Monthly Notices*, which contains shorter communications, abstracts of papers, lectures, &c., is given gratuitously to purchasers of the quarto volume. The two publications are supplementary to each other, and are to be considered as parts of the same series. They contain scarcely anything in common, except the Annual Report; and between them include a complete account of the proceedings of the Society during the year.

On the Construction of New Lunar Tables, and on some points in the Lunar Theory depending on the Conformation of the Moon with respect to its Centre of Gravity. By Professor Hansen.

(*Letter to the Astronomer Royal.*)

"As regards the Lunar Tables, I am now so far advanced with their construction as to be in possession of tables which represent the individual observations of the moon with a great degree of accuracy, and leave hardly any greater outstanding differences than those which are usually met with in observations of the fixed stars. After the correction of the originally assumed elliptic elements and of those coefficients of perturbation, the values of which can be ascertained *only* by observation, the following Greenwich observations were compared with the tables. In this operation there was no selection of the observations, those actually employed having been arbitrarily assumed beforehand, without knowing what might be the result of the comparison. Moreover, no further comparison of Greenwich observations with the tables has been instituted *subsequently* to the correction of the elements, so that the following results are unaccompanied by any circumstance which might tend to produce a bias in their favour.

1854MNRAS..15....1H

“ The outstanding differences between the observations and the tables (theory—observation) are the following, in which α denotes the right ascension and δ the declination.

Date.	Δα cos δ.	Remarks.	Δδ.	Remarks.
	″		″	
1824, Mar. 8	0.0		+1.1	
9	−2.9	... ...	−0.6	
12	+0.2	... ...	+6.8	
13	+2.1	... ...	−0.8	
14	−0.9	... ...	+5.8	
16	+2.1	.. ...	−1.7	
19	−0.6	... ...	−0.7	
Aug. 31	+4.7	... ...	+0.8	
Sept. 1	+0.5	... ...	−2.5	
2	+0.7	... ...	−0.6	
4	−3.2	... ...	+2.4	
5	−1.5	... ...	...	
6	−3.5	... ...	−1.5	
8	−3.5		−5.2	
8	−3.9	... ...		
12	+5.4	... ...	+4.6	
13	+2.7	... ...	+1.5	
15	+0.7	... ...	+0.4	
1832, Mar. 8	−6.0	... ...	+1.5	Faint.
9	−3.1	... ...	+2.0	Pond's Polar Distances of 1832 have been all increased by 2″.2.
10	−3.5	... ...	−0.5	
11	−3.4	... ...	−0.6	
12	−0.3	... ...	+2.2	
13	−3.0	... ...	−0.2	
15	−4.2	... ...	−0.1	
17	−1.6		+0.9	
18	+2.2	F. S. ...	+2.0	
April 7	−2.3	... ...	...	
8	−1.5	... ...	−1.5	
9	+1.7	... ...	−0.6	
10	−2.2	... ...	+0.6	
11	−2.1	... ...	+1.7	
12	+2.0	... ...	+0.9	
13	+0.6	... ...	−0.4	
14	−1.1	F. S.	+5.2	
16	+2.6	... ...	−0.6	
17	+3.4	F. S. ...	−3.0	
20	+0.5	F. S. ...	−0.9	
July 4	−3.2	... ...	−1.2	
8	+3.8	... ...	+0.9	
12	+6.3	R.	+2.6	
14	+5.2	R.	+0.7	
16	+5.6	... ...	+5.0	
18	+2.4	... ...	...	
19	(+7.3)	Extr. faint. ...	−3.1	
20	(+10.4)	R. No star of compar.	+3.1	

1854MNRAS..15....1H

Date.		Δα cos δ.	Remarks.	Δδ.	Remarks.
		″		″	
1838, May	2	−2·5	Very cloudy ...	+0·8	
	3	+0·1	... ...	+2·0	
	4	−1·5	... ...	−0·7	
	5	+0·4	... ...	−1·5	
	6	0·0	... ...	−1·0	
	7	+0·9	... ...	+0·3	
	8	−0·6	... ...	−2·6	
	8	...	... ...	+1·2	
	11	−2·9	... ...	−0·3	
	12	−3·7	Cloudy ...	+4·3	Cloudy.
	14	−2·2	... ...	+2·3	Faint.
	15	+1·0	... ...	+3·6	
	17	+4·4	No star of comparison.	+4·1	Very faint and flickering.
	27	0·0	... ...	−0·1	
	29	+0·6	... ...	−1·6	
	31	−0·5	... ...	−0·8	
June	2	−3·7	... ...	+1·4	
	4	−2·3	... ...	+1·4	
	6	−2·8	... ...	+3·2	North not so good as south limb.
	6	...	... ...	−0·5	
	7	−0·3	... ...	−3·3	
	8	−5·1	... ...	+0·3	
	13	−1·3	... ...	+3·9	
1843, Aug.	4	−3·4	... ...	+2·8	
	5	−2·3	... ...	−2·4	
	6	−4·2	... ...	−1·4	
	6	...	... ...	+2·4	
	7	−3·1	... ...	+1·7	
	7	...	... ...	+0·8	
	9	−2·3	... ...	+0·8	
	9	−0·6	... ...	...	
	10	0·0	... ...	+0·5	
	11	−1·7	... ...	+0·2	
	12	−2·1	... ...	+2·1	
	18	−0·5	... ...	−2·1	
	20	−1·6	... ...	...	
	22	−2·8	... ...	...	
	31	−0·5	... ...	−3·2	Merely approximative.
Sept.	2	+0·5	... ...	+0·9	
	2	...	... ...	+2·8	
	3	+0·6	... ...	+2·5	
	3	...	... ...	+2·6	
	5	−1·8*	... ...	+1·1	
	6	−3·5*	... ...	−1·1	
	6	...	* Irregularity in the	−2·3	
	7	(−8·7)*	azimuthal errors.	−2·8	
	7	...	... ...	−1·9	
	10	+2·1*	... ...	+0·8	

1854MNRAS..15....1H

Date.			Δα cos δ.	Remarks.	Δδ.	Remarks.
			″		″	
1843,	Sept.	11	−1·1	... ...	+0·7	
		12	+1·4	... ...	+1·2	
		16	−0·6	... ...	−0·9	
		29	+0·6	... ...	−0·1	
		29	...	... ...	−2·2	
	Oct.	1	−2·0	... ...	+1·1	
		4	−1·8	... ...	−1·1	
		5	−1·5	... ...	+0·9	
		9	+6·0	... ...	−1·2	
		12	...	... ...	−1·0	
		14	+1·8	... ...	−2·0	
		14	...	... ...	−1·5	
		15	+3·4	... ...	−1·7	
		16	−0·6	... ...	−2·6	
		17	+1·3	... ...	−2·2	
		18	...	... ...	−1·2	
1844,	Mar.	26	−5·4	... ...	−1·6	Both observations made with great difficulty on account of clouds.
		26	...	... ...	−6·3	
		28	−2·4	... ...	−2·9	
		29	−2·1	... ...	−2·1	
		30	−0·9	... ...	−0·2	
	April	1	−0·9	... ...	+1·1	
		1	...	... ...	+1·6	
		2	−1·2	... ...	+0·5	
		2	...	... ...	+2·5	
		3	0·0	... ...	−0·5	
		5	−2·4	... ...	+0·4	
		6	+0·3	... ...	−2·6	
		8	+2·3	... ...	+0·1	
		8	...	... ...	−1·4	
		9	+0·5	... ...	+2·9	
		10	+0·1	... ...	+0·7	
1850,	Mar.	17	−4·5	Very faint.	...	
		18	−2·5	Very cloudy.	−6·0	Very faint.
		25	−5·3	... ...	−0·5	
		26	−4·4	... ...	+0·5	
		27	−2·0	... ...	+2·6	
		27	−1·7	... ...	−1·3	
		28	−0·7	... ...	+3·2	
		28	...	... ...	+0·8	
		29	−3·1	... ...	+3·0	
		29	...	... ...	+1·5	
	April	2	+3·4	... ...	+0·8	
		2	...	... ...	+2·3	
	Aug.	13	−0·9	... ...	−1·8	
		15	−0·6	... ...	+2·6	
		16	...	... ...	+0·5	
		18	−1·2	... ...	−1·8	

1854MNRAS..15....1H

Date.	Δα cos δ.	Remarks.	Δδ.	Remarks.
	″		″	
1850, Aug. 18	...	... ...	−0·8	
19	+0·8	... ...	−1·9	
19	...	... ...	+3·5	
20	+4·3	... ...	+1·1	
21	+5·3	... ...	+1·9	
21	...	... ...	+0·7	
22	+2·6	... ...	+0·7	
22	+2·3	... ...	+1·9	
23	+2·8	... ...	−0·3	
23	...	... ...	+0·4	
24	+3·2	... ...	+0·6	
26	−3·0	... ...	−0·5	
28	+2·7	... ...	−0·4	
29	−1·0	... ...	−0·8	
30	−2·3	... ...	−1·5	
Sept. 11	+2·5	... ...	+3·4	
12	−0·2	... ...	−0·2	
14	+0·1	... ...	+0·6	
14	...	... ...	+1·0	
18	−0·2	... ...	...	
21	+1·3	... ...	+0·8	
21	...	... ...	+0·2	
22	+1·6	... ...	−2·2	
25	−0·8	... ...	−0·5	
26	−1·4	... ...	−1·9	
27	−2·6	... ...	−3·0	

Remarks upon the observations of right ascension for the year 1832.

'In the early part of August the pivots of the transit were examined and found to be unequally, &c.

'In the reduction no notice is taken of any instrumental errors, &c.

'The personal equation between Taylor and Ellis = 0^{s}·003, which may be neglected.

"Nothing is stated respecting the observations F S and R. In these also the personal equation would seem to have been neglected.

'July 20, faint. The observer (12) could not observe the moon-culminating stars, the atmosphere being hazy.

"If we now exclude from the foregoing observations those which I have enclosed in parentheses, since they are obviously affected with errors, and calculate the sum of the squares of the outstanding deviations from the tables, we have,—

In right ascension,

Out of 139 observations the sum of the squares of the errors = 952″·1

In declination,

Out of 157 — — — = 690·4

1854MNRAS..15....1H

Whence

The mean error of right ascension = 2″·62
— — declination = 2·10

"If, now, in consequence of the foregoing remarks, we also exclude the observations of right ascension for the year 1832, there results in right ascension,

Out of 113 observations the sum of the squares of the errors = 676″·2;

consequently, the mean error of right ascension = 2″·44.

"These errors do not differ essentially from those which usually affect observations of the fixed stars. I may add, moreover, that it is stated, with respect to Burckhardt's Tables, that 167 observations give the sum of the squares of the errors of the tables = 4602″, which corresponds to a mean error = 5″·25.

"Besides the foregoing observations, a number of the first observations of Bradley were compared after the correction of the elements, whence there resulted the following differences between the observations and the tables:—

Date.	$\Delta\alpha \cos \delta$.	$\Delta\delta$.
1751.	″	″
Oct. 24	+1·4	− 1·5
25	+3·5	− 5·2
26	+1·4	+ 3·0
28	−0·3	+ 9·2
29	−0·4	− 0·3
30	−4·3	− 0·2
31	−1·4	+ 1·2
Nov. 1	−1·3	− 0·2
2	+7·3	− 4·0
2	...	+ 0·7
3	+7·8	+12·1
7	+1·6	− 7·0
8	+3·8	− 3·6
9	−4·5	+ 3·0
10	+1·9	− 1·7
12	−4·1	...
13	+3·5	...
14	−1·8	...
1752.		
Feb. 22	−3·9	+ 5·0
23	−7·5	+ 2·5
25	+0·2	+ 3·9
26	+0·2	+ 5·9
27	+1·7	+ 5·3
28	+5·0	+ 7·7
Mar. 1	−1·2	+ 3·8

Date.	$\Delta\alpha \cos \delta$.	$\Delta\delta$.
1752.	″	″
Mar. 2	−1·8	+ 4·0
4	−1·5	+ 3·8
6	−6·6	+ 6·1
7	−3·7	+ 3·4
7	...	+ 2·1
9	+0·7	+ 3·7
11	+3·7	...
Oct. 11	+4·6	...
12	−3·5	+11·2
13	−2·7	− 6·0
16	−6·3	+ 2·4
17	0·0	+ 1·2
18	−2·5	− 1·8
20	0·0	− 5·6
21	+7·9	−10·7
23	−0·5	− 8·4
24	−4·3	− 8·4
25	−0·2	− 7·7
27	+5·3	− 4·4
28	0·0	+ 2·4
29	+0·9	+ 1·8
30	+7·7	− 9·1
31	−2·4	− 4·7
Nov. 1	+2·6	...
2	+2·2	...

1854MNRAS..15....1H

Date.	Δα cos δ.	Δδ.
1753.	″	″
May 6	+0·2	+11·9
7	(−10·5)	+12·9
8	−2·3	...
9	−1·5	+ 5·8
10	−0·5	+ 6·3
11	+1·7	− 1·1
12	+7·3	+ 5·0
14	+6·7	+ 4·3
May 15	+6·9	+ 5·7
16	−0·6	+ 6·6
22	+3·1	+ 0·4
23	+0·3	+ 0·1
24	+0·1	− 3·2
25	+5·2	− 4·0
26	+3·7	− 4·4

"Excluding some observations of right ascension enclosed in parentheses, we hence deduce :—

Out of 62 observations of right ascension, the sum of the squares of the errors .... = 884″·3
Out of 57 observations of declination .... = 1841″·0

whence,—

The mean error of a right ascension .... = 3″·78
— — — declination .... = 5″·70

Moreover, these mean errors are not greater than one might expect of such observations, when we take into consideration the observations of the fixed stars made at the same time, and bear in mind that Bradley then employed non-achromatic telescopes and the iron quadrant.

"I have also compared with the tables a series of Dorpat Observations of the moon, which furnish very beautiful results. But as there is a circumstance connected with them with respect to which I must ask the permission of our friend Struve, I will defer their publication for the present.

"I will now give the elements of the moon's orbit which I have obtained.

For the epoch,
1800, January, 0·0ʰ M.T., Greenwich.

"That is to say, for mean Greenwich noon of the 31st of December of the year 1799, there were found,—

° ′ ″
Mean longitude of the moon = 335 43 26·71
Longitude of the perigee = 225 23 53·06
Longitude of the ascending node = 33 16 31·15

"The following are the tropical mean motions in 100 Julian years :—

° ′ ″
Of the mean longitude = 307 53 39·61
Of the perigee — = 109 3 2·46
Of the node — — 134 8 59·61

with respect to which I would also remark, that, although I have reason to suppose that these motions of the perigee and the node very nearly represent the true values, still I do not give them as

1854MNRAS..15....1H

definitive results. It is besides a very difficult matter to determine these two motions accurately to a couple of seconds, since the effects of their variation upon the moon's longitude and latitude appear to be diminishing. I will now institute a comparison between these motions and those which you have found.

" From your paper, ' Corrections of the Elements of the Moon's Orbit, &c.,' the following results are deducible :—

Motion in 100 Julian Years.

		°	′	″
Of the longitude	=	307	53	21·0
Of the perigee	=	109	3	57·5
Of the node	=	—134	8	14·2

and in the paper, ' On the Eclipses of *Agathocles, Thales,* and *Xerxes,*' there is to be found,

		°	′	″
Motion of the perigee	=	109	3	17·4
Motion of the node	=	—134	8	54·6

" You see from this that the motions of the perigee and node last given by you agree very nearly with the values which I have found. I must, however, add this remark, that the observations comprised between the years 1820 and 1850 bear an increase of the value of the motion of perigee found by me, less than a diminution of the same value. By such a diminution the mean errors found above might be still further diminished.

" Between the motions of mean longitude found by me there exists a greater difference, but this arises from the circumstance that I have slightly altered the coefficients of the two inequalities of long period. The accurate determination of these two inequalities by theory, is the most difficult matter which presents itself in the theory of the moon's motion. I have on two occasions, and by different methods, sought to determine their values, but I have obtained results essentially different from each other. I am now again engaged with their theoretical determination by a method which I have simplified, and hope to bring the operation to a definitive close. I have also applied to my tables some coefficients which are not free from empiricisms, but which I can justify by the circumstance that they represent the ancient as well as the modern observations with great exactness ; and it may be expected that they will represent the future observations equally well. It is, moreover, an easy matter to replace these two coefficients in the tables by others, should this be deemed necessary.

" I have found the following value of the eccentricity of the lunar orbit to be employed in the table for the equation of the centre :—

$$e = 0{\cdot}05490307.$$

" I have also found, for calculating the principal term of the latitude, the following value of the inclination :—

$$I = 5^\circ\ 8'\ 40''{\cdot}21.$$

1854MNRAS..15....1H

"The coefficient of the parallactic equation I found to be,—

$$125''\cdot705;$$

an amount exceeding any which has hitherto been assigned, and which indicates a greater value of the sun's parallax than has been deduced from the observations of the transit of *Venus*. The Greenwich observations, exclusive of any others, assign the foregoing value of the parallactic inequality, and the Dorpat observations nearly the same value. I cannot, therefore, alter it. I may remark further that I have taken into consideration, not only the coefficient of the parallactic inequality itself, but also the largest of the remaining terms depending on the sun's parallax, and that the aggregate of these terms may amount to the fifth part of the coefficient of the parallactic inequality.

"The coefficients of the inequalities of the moon, depending on the [terrestrial] ellipticity, were found to have the following values:—

In Longitude = $7''\cdot624$; in Latitude = $8''\cdot382$.

"Add to these the terms, which I have deduced from theory, depending on the variation of the obliquity of the ecliptic, and there result in Longitude the terms,—

$$7''\cdot760 \sin(\Theta + 184^\circ\ 42') + 0''\cdot000157\ (t - 1800) \cos(\Theta + 260^\circ\ 5');$$

in north latitude,—

$$8''\cdot764 \sin(v + 169^\circ\ 51') + 0''\cdot00038\cdot8\ (t - 1800) \cos(v + 278^\circ\ 39'),$$

where Θ signifies the supplement of the ascending node, and v the longitude of the moon in its orbit. The logarithm of the ratio of the moon's semidiameter, to the sine of the equatoreal horizontal parallax, was found to be as follows:—

For the horizontal semidiameter = $4''\cdot750{,}484$
For the vertical semidiameter = $4\cdot75{,}0554$.

"It is known that different telescopes assign different diameters to the stars. Now, since at Greenwich the right ascensions and declinations have been observed with different instruments, it follows that the vertical and horizontal semidiameters of the moon must be deduced independently from those data. But even in the case wherein these two polar co-ordinates have been observed with one and the same instrument—for example, the meridian circle—it becomes necessary, when the observations refer to the moon, to execute this two-fold process. For we must bear in mind that the observations are made in different ways; since the observation of declination is made by bringing the limb of the moon in contact with the horizontal wire of the instrument, or bringing it midway between two horizontal wires, while the right ascension is found by noting the times of the entrance of the moon's limb upon the vertical wires. It may very well happen, from the different modes of observing, that the hori-

1854MNRAS..15....1H

zontal semidiameter may be found to be different from the vertical, even in the case wherein both of them are equal—a supposition, however, which has no foundation in theory. Nay, I have found, while engaged in determining the moon's orbit, indications that the personal equation between two observers may be different for the sun and moon from what it is for the stars, and that it even betimes may be different for the preceding and following limbs of the moon. I have not in the mean time been able to take into account this difference in the Greenwich observations, but I feel convinced that were I to do this, the agreement between the observations and the tables would be still better than it is. The difference between the above assigned values of the horizontal and vertical semidiameter is, moreover, very small, amounting only to 0″·15. Besides the above-mentioned quantities, determined by observation, which enter into my Tables of the Moon, there are two others of which I shall speak further on, after I have given some account of the mode of constructing the Tables, and of the method employed in determining the elements.

"The perturbational coefficients employed in the construction of the tables are, with the exception of the few which can only be determined by observation, those which I have calculated from theory. They are, throughout, accurate to two hundredths of a second: many of them I consider to be still more accurate. They already exhibit sensible differences from those of Damoiseau and Plana: nay, many of them differ from the corresponding terms of these tables to the extent of 2″ and 3″.* I have not yet published the perturbations of longitude and latitude, but the expression in the tables for the moon's parallax is, with a few trifling alterations, that which I made known in No. 403 of the *Astronomische Nachrichten.* The good agreement of the tables with the observations is a proof *à posteriori* of the accuracy of my perturbational coefficients, and of the soundness of the groundwork upon which the tables rest. I have applied to the latter, not only the larger coefficients, but also the smaller ones, which, indeed, are individually very trifling, but which, in their aggregate, exercise a not inconsiderable influence on the moon's place. Moreover, I have taken into account, not only the tenths, but also the hundredths of a second, since the latter produce a sensible effect upon the sum of many terms, and if they were not included, this sum, and consequently the moon's place itself, would frequently come out sensibly erroneous by the tables. Further, the secular variations of the perturbational coefficients, which are of sensible magnitude, are considered in the tables: their effect in 100 years may amount to 3″.

"The mean motions of the perigee and node above assigned are those which have been deduced from the observations, although they admit of being rigorously found by theory. But the ex-

---

* The sum of the differences between Plana's coefficients and mine amounts to 52″.

1854MNRAS..15....1H

tremely accurate determination of them by theory, as their application to ancient and all other observations demands, is an operation of such magnitude, that it will be always preferable to deduce their values from the observations. There will, therefore, be found in the tables, with reference to these two quantities, an empiricism, similarly as in the case of the two inequalities of long period, of which mention has already been made.

"For the sake of simplicity, tables with double arguments are employed in entering the smaller terms of perturbation, and, as in the solar tables, these are so arranged, that, in practice, it will be necessary to perform only one interpolation upon each of them individually, the second interpolation being executed upon the sum of all the terms. The peculiar form of the arguments of the lunar perturbations imparts to these tables a greater degree of simplicity than that which characterises the solar tables, since, in most cases, they admit the employment of double arguments, whereby the extent of every such table is reduced to half the dimensions which otherwise it would be necessary to give to it.

"Since the most frequent use of lunar tables consists in this,— that they shall serve to compute the moon's place for a series of equal, or nearly equal, intervals of time, the arguments have been expressed, not in degrees, but in time; and since the next application of them consists in comparing with these results a large number of meridian observations of the moon, the unit assumed in the arguments is the mean time which elapses between two successive culminations of the moon, in the same way as in the solar tables the mean day is chosen as the unit of the argument. By this arrangement the comparison of the observations with the tables will be very much facilitated. In order to compare the observations of a month, we select any whatever of the mean solar times of the observations, and calculate the corresponding values of the arguments from the tables. With this argument, increased or diminished by the units, 1, 2, 3, &c., we calculate from the tables the corresponding places of the moon. These are available now for all times which approach very near the times of the moon's culmination, and by a process of interpolation which it is easy to execute, since the interpolation factor is small, we may then transfer the calculated places to those which refer to the mean times of the meridian observations. The accuracy of the greater part of this calculation can be verified by differences, and only in respect to a small part of them will it be necessary to employ any other test. By the aid of the same tables we may also calculate an ephemeris of the moon, giving the moon's place from day to day for the same mean times as is done in the *Nautical Almanac* and other similar works. Still in this case the labour will be considerably increased, since the arguments vary throughout each day, not by a unit, but by fractions of a unit. I would prefer for such an ephemeris tables in which the unit of the argument was a mean solar day. This mode of computation is not, by any means, so laborious.

1854MNRAS..15....1H

"For determining the corrections of the elliptic elements, &c., the following process was employed.—With the exception of the mean motions, these corrections were deduced entirely from the observations of our own time, the ancient observations being employed only in determining the mean motion. The reason for this mode of procedure is, that the more recent observations are available in much greater abundance than is necessary for our inquiry; that they are more accurate than the ancient observations, and, therefore, must lead to more accurate results than the latter; and that, even if the ancient observations were included in the investigation, the value of the final result would not be enhanced thereby, inasmuch as they are less precise than the modern observations.

"For every observation employed in this part of the inquiry the corresponding place was calculated from the tables, and this calculation was carried on to the observation itself. The right ascensions and declinations of the observed limbs were therefore calculated, and the corresponding observations subtracted from them, whereby the influence of the errors of the assumed provisional values of the elliptic elements, &c. was brought out. Now, since by one of the foregoing preliminary determinations, the provisional elements were very nearly correct, it was found that the differences between the computed results and the observations were already very small. In order to diminish these as much as possible, the coefficients of the variations of the elements to be corrected were calculated for each observation, and the equations of condition formed. The number of unknown quantities in these equations was eleven; but besides, the coefficients of the variations of the three mean motions were calculated, annexed to the equations, and retained in the subsequent calculations, with the view of being enabled thereby to ascertain the influence of a variation of the provisionally assumed mean motions upon the remaining unknown quantities.

"The equations of condition were now distributed in groups according to the magnitude of the coefficients of the different unknown quantities, and all the equations of each group added together.

"For example, in determining the mean longitude of the epoch, the eccentricity, and the longitude of the perigee of the epoch, the observations for every 30° of anomaly were grouped together, whereby twelve equations were obtained; and a similar process was employed for the other unknown quantities. Hence resulted a large number of equations, which were treated by a process similar to that which the method of least squares leads to. Namely, the above equations, which have been cited by way of example, were multiplied and added; first, with the coefficients of the variations of the mean longitude, then with the coefficients of the variations of the eccentricity; and, finally, with the coefficients of the variations of the perigee. Hence resulted three final equations; and the other equations, treated in a similar way, added eight

1854MNRAS..15....1H

more to these; so that the number of equations was the same as that of the unknown quantities, and the values of the latter were obtained by the solution of these equations. The final equations to which one is conducted by this process do *not* possess the property that the coefficients in the vertical rows are relatively equal to those in the horizontal rows, as is the case with the final equations to which the method of least squares leads; but I can prove that they must necessarily possess this property *nearly*, and consequently that the cofficients in the rows referred to must be *nearly* equal to each other, since in each combination of groups all the corresponding equations of condition were employed. Nay, if even by this process the unknown quantities were not so determined that the sum of the squares of the remaining errors will be a minimum, still the result will be that this sum will approach very near to the minimum; and this will be sufficient in the present investigation, on which the rigorous employment of the method of least squares would entail a frightful amount of labour.

"I now come to a circumstance which has not yet been taken into consideration in the lunar theory. The remarkable circumstances which distinguish the motion of rotation of the moon necessarily imply a peculiar condition of the matter of which the moon's mass is composed; and theory replies in relation thereto, that the moment of inertia with respect to the principal axis, which is nearly parallel to the radius vector, must be the least; and that the moment of inertia with respect to the axis of rotation must be the greatest, of the three moments of inertia of the mass of the moon. The next assumption relative to the figure of the moon, and which is based upon this principle, is, that it is an ellipsoid, whose greatest axis is nearly parallel to the radius vector. Observation, however, has not hitherto confirmed this assumption. If, for example, this axis was to one of the other two in the proportion of 21 to 20, then upon this ground, and in virtue of the libration, there ought to be perceptible in the moon's semidiameter a variation of 2″. I have been unable, however, to establish any variation of this kind from the observations of the moon's diameter, which are available; and if even, in fact, a variation of the axis of the moon did exist, it must be much less than that deduced from the proportion above stated. Under these circumstances, it only remains that we assume that the interior of the moon is heterogeneous; and hence results the difference of the moments of inertia, which complicates the theory of the moon's rotation. There next arises, in connexion with this circumstance, the question whether, as in the case of the planets, the centre of the moon's figure coincides with the centre of gravity, as has been always assumed, or whether the positions of these two points may not be different? If the latter condition should really exist, then there may be found many laws of the density of the interior, in virtue of which the moment of inertia relative to the radius vector which is nearly parallel to the principal axis, will be the least of the

1854MNRAS..15....1H

three, even if the figure of the moon were different from that of a sphere.

"While engaged in the determination of the elements of the moon's orbit, of which mention has been made above, I undertook to examine this question, and I have ascertained as the result of my inquiry, that the observations of the moon entirely concur in indicating a difference between the positions of the two points above referred to.

"Let α, β, γ, be the three co-ordinates of the centre of the moon relative to its centre of gravity, so that α is nearly parallel to the radius rector, β perpendicular to it in the equator, and γ perpendicular to both of these; then theory shows that α and γ may be very well determined by the observations; but β, on the contrary, can be determined only with great difficulty, or not at all. The coefficient of β is literally down to small quantities equal to the coefficient of the variation of the longitude of the perigee, and the effect of β consists, therefore in great part, only in this, that the longitude of the perigee is increased or diminished by a constant quantity. For γ the Greenwich observations give $-1''\cdot 01$: and this quantity has been determined by itself with great certainty; only I must here remark a circumstance, which possibly may exercise an influence upon the magnitude of these quantities. It is known, that notwithstanding all the care bestowed in clearing the declinations of the stars from instrumental errors, still very frequently the declinations determined at one observatory deviate from those determined at another observatory in one direction, either north or south; and it is clear that an error of declination of this kind, should it be present, would affect the determination of γ; and the numerical value obtained for that quantity is fundamentally the aggregate of the above-mentioned co-ordinate γ and the constant error of declination.

"I must leave undecided the question, whether in the case of the Greenwich observations such an error may be supposed to exist, and leave this matter to your own judgment, since, as Director of the Greenwich Observatory, you are best acquainted with everything relative to the observations made there; but I could not refrain from a general allusion to the circumstance; nay, I even felt a desire to mention it to you.

"The co-ordinate α admits of being determined by the libration of the moon, and I have arrived at the following remarkable theorem relative to it:—

"*If the centre of gravity of the moon and the centre of its figure do not coincide, then must all the coefficients of perturbation for the mean longitude be multiplied by a constant factor, which is a function of the distance between these two centres projected upon the radius vector. If the centre of the moon be farther removed from us than the centre of gravity, then is this factor greater than unity; but if, on the contrary, the former*

1854MNRAS..15....1H

be nearer to us than the latter, the factor will be greater than unity.

"The observations enable us to decide that this factor is greater than unity, and that, consequently, the centre of the moon's figure is different from its centre of gravity, and lies nearer to us than the latter. I have given myself much trouble in applying the greatest possible certainty to this determination; I have executed it in many different ways; and on each occasion I have found, from the Dorpat as well as from the Greenwich Observations, that this factor is greater than unity, even although its absolute value, which could not be otherwise expected, exhibited a slight difference as it resulted from the various determinations. The final determination gave this factor—

$$= 1{\cdot}0001544;$$

and hence there results, among other consequences, an enlargement of the coefficient of the evection equal to 0″·69, and it is easily recognised that the enlargement of the sum of all the perturbations may exceed 1″.

"Esteemed Friend and Colleague! you have found by your discussion of the Greenwich Observations, extending from 1750 to 1830, that the principal coefficients of the lunar perturbations ought to be increased. You have found the increase of Plana's coefficient of the evection to be = 1″·28, and the increase of his coefficient of the variation to be = 0″·68; now, since the coefficient of the evection is almost double that of the variation, these enlargements seem to indicate the presence of a constant error.

"The increase of these coefficients which you have found is in each case greater than that which I have deduced, only I must here remark that Plana's coefficient of the evection is 0″·34 less than mine; and this circumstance leads to a closer agreement of our results. I would remark further, that you have found that Plana's coefficient of the annual equation ought to be increased by 1″·07, but your coefficient is too small by 1″·1, and therefore the extent of the increase will be considerably diminished.

"Permit me, in conclusion, to offer a few remarks on the foregoing explanation of the enlargement of the coefficients of the lunar perturbations. From the above-cited value of the factor it follows, that the centre of the moon's figure lies about 59,000 mètres; that is, about 8 geographical miles (reckoning 15 miles to a degree of the equator) nearer to us than the centre of gravity; and hence it follows, that between the two hemispheres of the moon, of which the one is directed towards the earth and the other is turned away in the opposite direction, there must exist a considerable difference with respect to level, climate, and all other circumstances depending thereon. Since the strata of homogeneous density must arrange themselves nearly in relation to the centre of gravity, it follows, if we suppose the figure of

1854MNRAS..15....1H

the moon to be a sphere, that the centre of the visible disk of the moon lies about 59,000 mètres above the mean level, and the centre of the opposite hemisphere almost as much under the same level: I say 'almost,' since if, as we here must assume, the opposite hemisphere of the moon is more dense than the hemisphere turned towards us, it necessarily follows that the mean level of the former will be somewhat depressed, and the mean level of the latter somewhat elevated. If we suppose the moon to be an ellipsoid, the elongation of which lies in the direction of the earth, then the hemisphere of the moon which is next the earth will rise a little more above the mean level, and the opposite hemisphere will sink a little more beneath it. Nay, we may consider it as not impossible that the surface of the opposite hemisphere of the moon wholly or partially accommodates itself to one and the same level, in a similar way as we find to be the case with the earth.

"We need not, then, under these circumstances, wonder that the moon, when viewed from the earth, appears to be a barren region deprived of an atmosphere, and of all animal and vegetable life, since, if there existed upon the earth a mountain proportionally high, and, consequently, having an elevation of 216,000 mètres, or 29 geographical miles, there would not be recognisable upon its summit the slightest trace of an atmosphere, or of anything depending thereon. We must not, however, conclude that, on the opposite hemisphere of the moon, the same relations exist; but rather, we should expect, in consequence of the distance of the centre of figure from the centre of gravity, that an atmosphere and animal and vegetable life may there find place. Nearly at the moon's limbs the mean level must exist; consequently, we might reasonably expect to discover there some trace of an atmosphere.

"If we now inquire into the cause of this condition of the moon, I hold it to be not impossible that volcanic and other similar forces in the interior of that body may have met with far less opposition in one of the hemispheres than in the other, and may therefore have effected much greater upheavings of the surface in the former than in the latter. I am also disposed to think that the phenomena termed 'Rills,' which are perceptible on the moon's surface, and respecting which selenographers do not seem to have yet, upon the whole, arrived at any satisfactory conclusion, are rents or splits, which have been occasioned by these enormous upheavings. I submit to the judgment of astronomers these considerations, which, strictly speaking, do not belong to the theory to which this letter is devoted, but which originate in the distance of the centre of gravity of the moon from its centre of figure.

"The theory of the moon's figure, which leads to the theorem above cited, as well as to several other conclusions, has been developed by me in a Memoir which I have the honour of herewith communicating to the Royal Astronomical Society.

"*Gotha*, 1854, *Nov.* 3."